With Dr. Crombie's compliments.

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PRESIDENTIAL ADDRESS
IN
MEDICINE AND PATHOLOGY.

BY

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PRESIDENTIAL ADDRESS IN MEDICINE AND PATHOLOGY.

By Surgn.-Lieut.-Col. Alexander Crombie, m.d., i. m. s.

Gentlemen,—While I acknowledge the great compliment which was paid to me by my nomination to the distinguished position of President of the important section of Medicine and Pathology at this the first Medical Congress held in this country, I trust you will accept, not as merely formal and customary, but as genuine and heartfelt, my expression of regret that some one has not been selected who might have filled the place I now occupy with more learning, grace, and ability than I can hope to bring to your service. In order to minimise as far as in me lies the disqualifications under which I take my place here, I have selected for my presidential address a subject which has an instant and perennial and daily interest for all of us who practise medicine in India, and one to which I have given much thought throughout my whole service, and the importance of which no one will be disposed to dispute. It is now nearly twenty years since I was requested, among other officers serving in Burma at that time, to report on the fevers of that country, and from that time till now my mind has never been long unoccupied with the great important and complex problem which Indian fevers present. To the European living in Europe on account of the almost theatrical rapidity of its course, its high rate of mortality, and the mysterious way in which it seems periodically to escape from the limits of the endemic area, and carry devastation westwards in a progress which neither rivers, nor seas, nor quarantine are able to stop, cholera looms large in the catalogue of Indian diseases. But we who live and practise in India know that in many parts cholera, as in Europe, is only an occasional visitor, and that even in the endemic areas, where though it has its seasons, it is never wholly absent, while it demands its victims in hundreds, fever exacts them in thousands. The number of deaths due to cholera in the whole of India in 1892 was recorded roughly as 750,000, whereas the number of deaths attributed to fevers was, also roughly, 4,500,000. Now if we remember that the death-rate in fevers is probably highly estimated if we place it at two per cent., while the death-rate in cholera is not much below 50 per cent. of the cases, we at once perceive what an enormous amount of sickness, suffering, and disability is comprehended in the words Indian Fevers, and what an enormous amount of clinical material there is presented to us yearly for their study. The above figures, worked out, give us somewhere about 200,000,000 cases of fever per annum, against 1,500,000 cases of cholera. The pre-eminent importance of fevers in the daily life of an Indian practitioner can, therefore, hardly be disputed, and I need, I think, make no excuse for selecting them as the subject of my address.

The fevers of India divide themselves into two great classes or groups :—In the first of these are those fevers whose course is interrupted by more or less perfect periods of apyrexia, and are, roughly speaking, amenable to treatment by quinine. These are the malarial fevers, and in the second group are the continued fevers, in which quinine is of no avail. With regard to the first, our knowledge is precise and almost complete; with regard to the second it is still hazy and to a large extent conjectural, and yet it is in this second group that are found the fevers, the fatal character of which goes so far to swell the death-rate of India to its huge proportions.

I will speak first of the fevers in which periodicity is the distinguishing character, namely, the malarial fevers. It is customary to subdivide these into intermittent and remittent fevers. With regard to intermittent fevers, we are all of one mind, and there is no question, and can be no question, as to their position, but of late years there has arisen a good deal of doubt, first, as to whether there is any fever which can be called remittent, and if so whether it should be included among malarial fevers. I confess to placing myself among the sceptics for reasons which will presently appear.

Beginning then with those fevers which in their normal or typical manifestations have distinct periods of perfect apyrexia, namely, the intermittent fevers, I would point out in the first place that they do not in India follow the same order of frequency as they do in other countries. In those parts of India in which I have had opportunities of observation quotidian ague takes the first place in order of frequency. At most seasons I would not probably be wrong in saying that 90 per cent. of the cases of ague we have to treat have daily paroxysms of fever and not more than 10 per cent. are tertian in type, while quartan ague is so rare that it may be practically left out of account. I have only had to treat one case of quartan ague in the whole of my twenty-two years' service in India, though I heard of another case a few weeks ago, but was unable to see it. In other malarial countries the order of frequency is quite different. In the Roman Campagna, for instance, the majority of the cases are tertian, then come quartan, and last of all quotidian agues. To this fact, the comparative infrequency of tertian and quartan ague, I attribute the reluctance which is still shown in some quarters in India to accept the amœba of Laveran as the true pathological cause of the symptoms of malarial fever, because in quotidian ague the amœba is often difficult to find, for the reason of its small size, its often indistinct form, and because it does not go through all the phases of its cyclical life in the blood of the general circulation. In tertian and specially in quartan ague it is of large size, well pigmented, and generally requires no great research to find it, and in quartan ague its whole life-history can be followed in blood taken at intervals from the finger. To study the most critical part of the cyclical life of the parasite in tertian and quotidian ague, namely, the periods of segmentation and sporulation, blood must be taken from the internal organs. From these remarks you will guess, and I may as well at once confess, that I am a convert to the amœbic theory of malarial fever. It seems to me that it is impossible to offer any longer a reasonable resistance to this theory. Malarial fever is found to exist in widely different parts of the world, in America, Africa, Asia, and

Europe, and in localities differing from each other in every particular of climate. Yet under all these varying conditions the phenomena of malarial fever are identical, and point to an identical pathological cause. In all these places, an examination of the blood of patients exhibiting these symptoms, discloses the presence of a living organism which is the same in each place, the same in Africa and America as in Bombay and Calcutta; furthermore, the cyclical growth and development of the parasite is found to correspond with the cyclical periodicity of the fever, the apyrexia corresponding with the appearance of the young amœbæ in the corpuscles and the accession of fever with their sporulation and the discharge of their presumed toxin into the blood. If it is to Laveran we owe the discovery of the relation of the amœba to malarial fever, it is to Italian physicians, and especially to Golgi, Marchiafava and Bignami, that we owe the working out of its life-history, and it seems to me impossible to resist their conclusions.

Golgi has made quartan ague his special study, and the ease with which the life of the amœba of that form of ague can be studied in the blood of the finger, makes it very enticing. Its cycle of life is shortly as follows, and I am glad to have this opportunity of saying that I was able, even with my very imperfect observations, here in Calcutta, to confirm those of Golgi and Marchiafava, as far as I could do so in the single case of quartan ague which came under my observation last year. In the first day of the apyrexia the amœba will be found, perhaps, two or even three in a single field of the microscope as in my case, occupying about $\frac{1}{4}$ or $\frac{1}{5}$ of the invaded red blood corpuscles and exhibiting active amœbic movements. On the second day of apyrexia it will be found that it has grown so as to occupy half of the blood corpuscle, to have become pigmented at the periphery and to have all but lost its motility. On the third day of apyrexia it will be found to occupy nearly the whole of the corpuscle, and that the pigment has collected towards the centre preparatory to segmentation. The next and last stage of the cycle of life is that of sporulation, and with the discharge of the spores, and of the toxin which the amœba is supposed to form comes the sudden accession of fever on the fourth day. When this happens over and over again, the same relationship being repeated between the growth and maturation of the amœba and the periods of apyrexia and pyrexia in the patient, it becomes irresistibly borne in on one that they bear an intimate and indeed causal relationship to each other.

The study is more difficult in tertian, and most difficult in quotidian ague, because the amœbæ of these forms of ague cannot always be found in the blood of the finger, retiring as they do to the spleen, brain, and bone marrow to undergo their final stages of segmentation and sporulation prior to a renewed accession of fever, and indeed it is sometimes only on *post-mortem* examination that the important part they have played in the phenomena can be understood, the number of amœbæ in the general circulation not being always proportionate to the severity of the symptoms. All that is wanted to satisfy the most exacting demand for complete evidence that the amœba of Laveran is the very cause of intermittent fever is, that it should be cultivated outside the body in some suitable, medium, and that when a pure culture so obtained is injected into the body of an animal susceptible to the disease it should give rise to the symptoms and pathological changes of intermittent fever. Hitherto the amœbæ of malarial fever have baffled every attempt of this kind if it be not true as was reported a few months ago, that Celli of Rome has succeeded in growing them in some alkaline medium.

It would require a much more prolonged and continuous study, than it has been in my power to give to this subject, before I could say or ask you to believe, with Marchiafava, that each form of malarial fever has its own variety of the parasite, and that you can tell, by its microscopic characters which variety is present and that the specimen of blood is taken from the finger of a patient suffering from quotidian, tertian or quartan ague as the case may be; but it is my firm belief that there is little difficulty in doing so, and my own observations here in Calcutta as far as they go, go entirely to confirm the observations of the great Italian observers as may be gathered from the phrases I have used in speaking of the parasites of the different forms of intermittent fever, as if they were distinct. I have now seen every form of the amœba both the endoglobular and the ectoglobular bodies belonging to the semilunar group with the exception of the flagellate forms in patients in the General Hospital in Calcutta, and they exactly correspond with what I saw in Rome, and is described and pictured in Marchiafava and Bignami's book; and not only so, but the variety described as that of quartan ague was found existing alone in my case of quartan ague I have alluded to. Here the commonest variety is the annular form found in our cases of quotidian ague exactly like that found in cases of quotidan ague from the Roman Campagna, and the same correspondence will be found also, I believe, in the tertian ague of the two countries.

I now pass to those cases of malarial fever in which there is no distinct intermission. These form an important proportion of the cases of fever treated under the name of remittent fever, and allow me to say at once that we all recognise two kinds of remittent fever, a malarial and a non-malarial, which latter I, however, regard as a continued fever; and will consider later as one of the group of continued fevers. But at a certain season of the year, that is, from the middle of September to the end of December in Lower Bengal at least, a very large percentage of cases of remittent fever partake of a different character, that is to say, they are clearly of a malarial nature, and are equally with the varieties of intermittent fever mentioned before, amenable to treatment with quinine. There is little room for doubt that this group of fevers the malarial remittents is also produced by the same amœba forms, as produce the intermittent fevers, and that they are in fact intermittent fevers in which either the pyrexial stage is unduly prolonged, owing to the excessive toxic qualities of the organism, so that a new accession of fever comes on before the previous attack has entirely subsided, giving time only for a small remission of the pyrexia previous to the next accession, or they are cases in which there are at one and the same time two sets of organisms in the blood whose cycles of life are parallel but not synchronous, one maturing, sporulating, and discharging its toxin some eight, ten or twelve hours after the other, and thus keeping up a continuous condition of pyrexia. Just as there is a double tertian, so there may be, and in certain of the cases I refer to there is a double quotidian producing the phenomena, which constitute what is described as malarial remittent fever. I have not made a sufficient number of observations to verify those of Marchiafava who has found corroboration of this theory of malarial remittent fever in the existence of amœbæ in different stages of cyclical growth in the same drop of blood, but I have found the annular forms of the amœba present in cases of remittent fever in no way to be distinguished from those so constantly found in the quotidian form of intermittent. I confess, however, that these are most trying cases to investigate microscopically, at least in the time usually at my disposal for these purposes.; and I have in most cases been obliged to form my diagnosis on general clinical symptoms.

We must, therefore, in many cases fall back on general symptoms in differentiating the malarial from the non-malarial form of remittent fever, and the clinical features on which we must depend to justify the diagnosis of an element essentially malarial in these cases are the following: (*a*) the history of a distinctly intermittent character of the symptoms at an earlier stage before they become remittent in type; (*b*) the existence of a more or less distinct remission of the pyrexia at a fairly regular time of the day or night; or (*c*) the occurrence of two remissions however slight, and two exacerbations in each period of twenty-four hours suggesting the existence of two sets of organisms maturing separately. Observations of this nature require the most intelligent care and record, but when one or other or all of these phenomena are present, you know, that you are dealing with a case of malarial poisoning, and you must exhibit the antidote even though no malarial organisms can be found in the blood of the finger.

One more word before I pass to the consideration of the group of continued fevers. It may be asked, what practical good is obtained by the discovery of the malarial parasite in the blood; the clinical phenomena are already sufficiently clear to enable us in nine cases out of ten to dispense with the microscope in forming our diagnosis? This is no doubt true, but not to mention the aid to prognosis afforded by the number of parasites present and their effect on the red corpuscles which they have invaded, there are cases in which the diagnosis is not quite easy, and in which the discovery of the parasite would free the mind of the physician from doubt and perhaps anxiety, and enable him to use the antidote with a freedom and confidence which he could not otherwise feel. Thus in my own practice last year I had a case of urinary infiltration which, while apparently doing well, was suddenly seized with rigors, and a rapid rise of temperature followed by profuse sweating, symptoms which, under the circumstances, were uncomfortably suggestive of pyæmia. An examination of the blood showed the presence of the amœba of tertian ague, and led to the administration of a few full doses of quinine, and the cessation of all the symptoms. I venture to think that without the microscope the treatment would not have been so assured or so prompt and effective. In the puerperal state similar assistance would often be of the greatest comfort to us. It is only fair in me to say, however, that in many cases the observer must be prepared for disappointment. The examination must often be prolonged and repeated, and there are various fallacies lying in wait for the inexperienced, and in certain instances where the general symptoms and the prompt response to the action of quinine leave no ground for doubt as to the malarial character of the case, the most patient search sometimes fails to find any organism in the blood of the finger. In such cases the blood of the spleen would probably yield them abundantly. While, therefore, the presence of the malarial parasite in the blood of the finger may be regarded as conclusive of the malarial nature of the case, failure to find it can in no way justify the opposite conclusion in the face of distinct clinical symptoms of a malarial type.

And here pardon me one moment, that I may speak of the efficient use of quinine. There are many practitioners in this country who, from timidity or wrong teaching or some other cause, have no knowledge of, and I fear also no belief in, the power of quinine as an antidote for malarial poisoning. How, when, or where the practice originated I cannot discover, but the belief is widespread, among professional men as well as among the laity, that quinine cannot be safely or efficiently administered except during the period of apyrexia, or at least in remittent fever its administration must be delayed till a remission has occurred such as to bring the temperature down to some point below 100° F. All the best authorities advocate a very opposite plan of treatment, and I am glad to have this public opportunity of recording an emphatic dissent from the popular belief and practice. Careful observations have shown that quinine is most efficient against the malarial organism in the early period of its growth, while it is still unpigmented or only collecting pigment at its periphery, *i.e.*, in quotidian ague during the pyrexia and the crisis. When the amœba ceases to grow and begins to prepare for segmentation and sporulation, that is, just before the rigor, and new accession of fever, quinine has little or no effect on it, and I entirely agree with Marchiafava and Bignami, that in malarial remittent, where there may be, and frequently are, two sets of organisms maturing separately and severally, at different times, in the blood, the treatment with quinine should be begun without delay and continued at regular intervals without any regard to the state of the temperature curve. My own practice is to give 10 or 15 grains by the mouth, every four or six hours according to the severity of the symptoms.

I have never given more than 90 grains in 24 hours, and I usually find one drachm in 24 hours serves my purpose, but in the hospitals of Rome where they encounter a type of fever of a severity such as we rarely meet with here, and which may prove fatal by coma in 48 hours, physicians administer much larger doses. Their initial dose is seldom less than 30 grains, in two doses at an interval of two or four hours, and in pernicious cases they give hypodermic injections of 30 to 45 grains, followed by smaller doses of 15 grains.

If when the time of my leaving India comes I were to leave a message to my professional brethren here it would be this,—treat your cases of malarial remittent with quinine early in full doses with a free and fearless hand and regardless of the temperature.

Gentlemen, I yield to no one in my advocacy of the freest use of quinine in malaria, but at the same time I deprecate and deplore that indiscriminate use of it in unsuitable cases, which has brought this invaluable drug into so great disrepute among the Natives of this country, and has given rise to a dread of it both on the part of patient and practitioner, which effectually prevents its effective employment in cases where it is the only hope of salvation. In this instance, as in every other, a careful diagnosis is essential. If after a painstaking enquiry the case appears to be one of the second group of which I am about to speak, the group of continued fevers, withhold quinine or give it sparingly and tentatively, but if you come to the conclusion that the case is malarial, you cannot give it too freely or fearlessly or with too lavish a hand.

I now come to the second group of fevers, our knowledge of which with, perhaps, one exception is unfortunately still vague and conjectural, and in which quinine is not only of no avail, but is sometimes distinctly harmful. I refer to the continued fevers of which I recognise with certainty three, and probably five, as specifically distinct. The three whose differentiation I regard as indisputable are simple continued fever, typhoid fever, and non-malarial remittent, so called,—the other two whose title to a distinctive name you may be disposed to doubt are a fever especially of towns known locally as " Calcutta fever," " Bombay fever," etc., which runs a course very similar to that of typhoid fever without any of the symptoms of that disease, and lastly, there is a low continued elevation of temperature of indefinite duration and without any distinctive symptomatic features and known simply as " low fever. "

I do not propose to allude to ephemeral fever of 24 hours' duration and due to chill or slight exposure to the sun, or to that extremely sudden severe and fatal form of fever called insolation or sun-stroke, or to cerebro-spinal or typhus fevers, but I would venture to remark that whatever the returns of the armies, both European and Native and of civil hospitals and jails, may seem to say to the contrary, the most common fever of India is not ague. The fact that "ague" takes the first place in the returns is merely due to a looseness of nomenclature which leads medical officers, at the instance generally of their subordinates to return as "ague" every case of fever of short duration, as well as those which show the intermittent character which entitles them to that name. I was a very short time in India before I began to fight on this subject a battle which continues even unto this day. To my mind no fever is entitled to be called ague which does not conform to a certain type, that is to say, unless its course is interrupted by intervals of more or less complete apyrexia.

Now in the parts of India where my lines have fallen a type of fever with this essential character has at no time or place been the prevailing one as the returns referred to would lead one to suppose, and if you were to refuse to accept as ague all cases of fever except those in which intermission was an essential feature, you would sweep away at once some three-fourths of the cases now returned under that name. In certain months of the year, that is to say, in October, November, and December true agues are more frequent than at other seasons, but even then they by no means constitute the majority of the cases, and for nine months of the year it is often difficult even in a large hospital to find a case of genuine ague for weeks at a time.

The cases which are thus wrongly returned under the heading of "ague" are really cases of "simple continued fever," a fever often ushered in with a rigor, a very high temperature and evidence of great gastric disturbance. I have known the temperature reach 106° F. within a few hours of the commencement of the symptoms, but more usually the ingress is more gradually, though the headache and gastric symptoms are generally very distressing. The duration of the case is generally from three to eight days, and there is often much doubt whether it will or will not pass into typhoid when the patient is a European, and especially if he is living in a drained town.

Such cases are examples of a distinct type of fever which has been recognised and described by all the best authorities under the name of simple continued fever a name to which there is no objection. But instead of determining within a week it is not very unusual, especially during the cold season, and early summer for these cases to continue for a longer time, and then we look for defervescence about the 14th, 21st or 28th day. These cases are particularly common in towns and are known as "Calcutta fever," "Bombay fever," etc. It is open to any one to assert that these are cases of mild typhoid fever, and that those which recover early are merely abortive cases of that disease. It is, however, I think impossible for one accustomed to treat considerable numbers of cases of true typhoid fever, and who also has opportunities of watching the imperceptible gradation of cases of simple continued fever into the cases now under consideration, not to notice distinct differences which forbid him to concede more than that certain mild and abortive cases of typhoid may be mistaken for cases of the continued fever of our towns. I am one of those who are unable to admit that the whole philosophy of continued fever in India can be epitomised in the word typhoid. I cannot admit that the bacillus of Eberth is the only organism capable of giving rise to a febrile curve of three or four weeks' duration. As a matter of fact, it is known and has been experimentally proved in animals that a bacillus normally present in large number in the human intestine, the bacillus coli communis, is capable under certain conditions of becoming virulent and then endowed with a power of producing a fever and even pathological appearances, including ulceration of the bowel, hardly to be distinguished from those of typhoid fever. It has always, therefore, seemed to me to be narrow and unjustifiable to classify as necessarily typhoid every case of continued fever of three or four weeks' duration. If the bacillus coli communis is sometimes endowed with this power, why not also some other organism or organisms as yet unknown? With the knowledge of this possibility before my eyes, I have been accustomed to insist on the presence of certain symptoms resembling those of typhoid fever before being prepared to concede that any given case of continued fever is one of that disease. Now such symptoms in the cases I refer to are conspicuous by their absence. It is true that genuine cases of typhoid fever often in this country present remarkable variations from what may be considered as typical of the disease described in text-books. I need only allude to the frequent absence of that gradual step-like rise of temperature during the first three or four days which is so characteristic of typhoid fever in Europe. Instead of that we quite frequently, and I believe it to be the rule here, have a sudden rise to 104° on the first and subsequent days. I need not mention the alleged more frequent absence of the characteristic eruption here, because I do not think it is more often absent here than in Europe, and even if it were it is not so much the absence of spots which constitutes our difficulty of diagnosing typhoid in India, but rather the superabundance of spots, at least in the hot weather and rains, when the whole body is covered with prickly heat, in the exuberance of which it is as hopeless to pick out typhoid spots as to find the proverbial needle in a bundle of hay. Neither do I speak of the difficulty of seeing the spots on pigmented skins, because as I have pointed out elsewhere the Native races of India exhibit a marked immunity from typhoid fever, with the exception of the lighter-coloured races, the Gurkhas, and Burmans, and perhaps other meat-eating and dram-drinking peoples. Neither do I speak of the relative frequency of constipation in typhoid fever in India, because I do not think it is more common than in Europe; and because I believe that the constipated motion of a genuine case of typhoid with its appearance of orpiment paint is as characteristic of the disease as is the pea-soup stools of typhoid diarrhœa. But what I maintain is that we are not justified in designating as typhoid fever cases which have neither the gradual initial rise nor the eruption, nor the characteristic stools whether loose and like pea-soup, or constipated and like yellow coach paint, cases in which the tongue continues clean and moist, or simply coated as in any febrile condition, and in which the mind is clear throughout unless you give quinine, I say we are not justified in calling such cases, in which all the symptoms of typhoid fever are absent, enteric fever simply because they have a duration of three of four weeks. I demand that some one or more of the ordinary symptoms of typhoid fever should also be presented. Neither will I admit that even hæmorrhage from the bowel necessarily removes them to that category if it be true that other febrile infections besides that of typhoid may also give rise to intestinal ulceration. I believe that these cases which I am now describing, and which are so sharply differentiated from typhold fever by the whole clinical picture which they present constitute a specific disease, and must have a specific and separate cause distinct from the bacillus of Eberth.

A very few words will suffice to dispose of the next form of continued fever. It is one which is only occasionally met with among Europeans, and I do not think I have met with it in natives of India, but if it does occur among them, it would hardly be brought under my notice. I refer to a persistent low elevation of temperature unaccompanied by any constant symptoms, of indefinite duration and uninfluenced either by quinine or arsenic. The temperature never falls below 99° and rarely rises above 101·5. It may continue for several weeks without complication except perhaps a tendency to diarrhœa of a bilious character, with loss of appetite and gradual loss of strength and flesh. Some of these cases are distinctly aggravated by quinine, and I have known them cease abruptly on withdrawing the drug which had been persistently given in the belief that the condition was malarial in its essential nature. These cases are spoken of as "low fever" and are generally cured by a "change" of any kind but especially by a trip to sea, and it is especially this form of fever which in Calcutta is benefited by a visit to the Sandheads. Though very ill-defined these cases constitute a distinct type of fever at once recognised when met with.

Widely different from this "low fever" is the last form of continued fever of which I have to speak, namely, the non-malarial remittent (so-called). It is a pity we have no better name for this fever which is of very frequent occurrence and is one of the most fatal of our fevers. The designation "remittent" is a misnomer. It is no more remittent, and indeed often less so than typhoid. The temperature is generally very high touching 104° and 105° for a long part of its course, and the daily fluctuation not exceeding 2° or 2·5°. It begins in a way not unlike the cases I have described under the name of "Calcutta fever," "Bombay fever," etc., and is by some considered to be a variety of typhoid fever, notwithstanding its divergence from all the symptoms of that disease. Hepatic enlargement and congestion are early and constant conditions, but the spleen, as a rule, continues impalpable below the ribs. Bilious diarrhœa, in no respect resembling the diarrhœa of typhoid, is also a very frequent symptom. Quinine often given in large and repeated doses in these cases is not only useless, but so obviously adds to the distress of the patient without in any way producing an improvement in the progress of the symptoms that it is very soon abandoned. Meanwhile the temperature continuing persistently high, marked head-symptoms, especially delirium of a muttering and irritable kind, comes on, and the patient may even and often does pass into a condition of coma from which he can hardly be roused. This condition, one of persistent high temperature without any marked remission, a distinctly enlarged and congested liver with bilious diarrhœa, congestion of the back of both lungs, and a low muttering delirium is generally reached by the 18th to the 24th day. If coma supervenes, the patient frequently dies about this period. In more favourable cases where the symptoms are less severe, they continue for a week or two more, and the average duration of the case is six weeks. None of our drugs seem to be of any use in shortening it, certainly not quinine, the inadequacy of which in these cases has now become fully recognised, though the disease has not yet found a name or place in our nomenclature.

Though I have seen this fever in Europeans, it is essentially a disease of natives, and is not common after thirty years of age, though frequent enough in childhood. Like every other continued fever in India this also has been called typhoid fever, though it is difficult to conceive a condition more distinct from it in its symptoms or course. This is the fever which offers the highest promise to the inquirer of the near future. It is obviously not a malarial fever, and no unprejudiced observer would for a moment class it with typhoid, and yet it has such a perfect and constant clinical course that it must have a special and specific cause, some organism which is waiting its discoverer. Such discovery has at last been made with regard to "Malta Fever," a fever long and persistently considered to be typhoid, but now proved by the researches and experiments of Bruce, Gipps and Hughes of the Army Medical Department to be as distinct clinically, pathologically, and bacteriologically from typhoid fever on the one hand, as it is from malarial fever on the other. It has been my fortune to watch some twenty cases of Malta fever in the General Hospital, and it is difficult to understand how it could ever have been regarded as related, even to typhoid fever so different is it in its clinical character alone. The gentlemen I have named have succeeded in isolating the organism which is its essential cause, and have been able in six instances to induce an analogous fever in monkeys by means of pure cultivations of the "micrococcus melitensis." As it is called, Malta fever has no congener in India, but should not the success of Bruce, Gipps and Hughes, stir us up to a scientific investigation of our Indian continued fevers? These investigations are not really difficult. Their essential requisites are a few simple stains and a patient looking through the tube of a microscope. We have allowed a Frenchman to find in this way for us the amœba of our malarial fevers and a German the comma bacillus of cholera, which is surely our own disease. Shall we wait till some one comes to discover for us the secrets of the continued fevers which are our daily study, or shall we be up, and do it for ourselves? It may be that I have wearied you with my address the subject of which is necessarily one of technical detail. It may be that you differ from me entirely in my opinion as to the nature of the fever which I have now attempted however feebly and imperfectly to differentiate and classify, but if I have succeeded in rousing any of my hearers or any of those who may do me the honour of reading my remarks to a more careful clinical study of the cases which come daily under our observation in this enormous field for clinical study, in which we work, and there must be many better qualified for the task than I am; and especially if I can hope to rouse in those who have more leisure than I, the determination to study these fevers bacteriologically and to make a precise knowledge of their true nature our own, I shall be content and think that I have perhaps not written and spoken in vain.

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